

THE PARADIGM OF THE RHINEAN SCHOOL

PART 2. THE CONCEPT OF SCIENCE

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A group of researchers share a similar view of their own activity as investigators, and also of the position of their science in the world of sciences. They have a common conception of how their discipline was born, developed, and what it will look like in the future. They also believe in certain rules for carrying out research.

Basic to the concept of science is the theory of knowledge, an understanding of the foundations of knowledge. However there are also normative conceptions of what science should be, what theories should look like, or which criteria one has to use in the search for truth. The normative part may be called the model of science. It is a value system. Investigators often look at a superior science and obtain their categories and perspective from this. Since the 17th century most investigators have used physics as a model, as it is supposed to treat the deepest level of reality. Therefore the history of psychology can roughly be depicted as a sequel to the physical sciences with a delay in time.

In parapsychology there have been a lot of theories and concepts modelled on physics. Obvious examples of physical analogies are aether-models, radiowaves and psi dimensions in space, but the S-R experimental model is also a physical model. Analogies have a heuristic value, as they put questions and prescribe new results. However they also narrow the perspective: the physical model - thinking in parapsychology has not lead to a better understanding of psi as a psychological process. At best it has given the field a scientific status.

A characteristic feature of a school-builder or paradigm-shaper is his occupation with problems of methodology and perspectives of science. This can also be said of Rhine. Both his books and his editorials in the Journal of Parapsychology are rich sources for one's understanding of his concept of science. From the 1930's

until today he has repeatedly discussed these problems and often modified his point of view.

As research progressed he has commented on the position of parapsychology. He regards the development of a science as a slow accumulation of facts and he can often announce an attainment of a milestone or a breakthrough in a certain area: 'Now we have enough of evidence to consider ESP proven, now it will suffice for the existence of PK'. When he began in the early 1930's at Duke he regarded himself as a pioneer. He was aware of what had been done earlier in the USA and England, and also to some extent in Germany and France, and some other European countries. But little of this research could withstand his criticism. He had to start from the beginning, fresh, and as he hoped, without any presuppositions as to the existence or nature of psi phenomena. In 1940 he was able to summarize the first step in the research programme: the existence of general ESP was settled (Rhine, 1940). The amount of 'facts' increased each year and Rhine attributed their meaning to the growth of scientific knowledge: Now we could acknowledge the dependence of psi on motivational factors, and now the existence of internal patterns in the ESP scoring, e.g. decline effects or U-curves.

Rhine has insisted more than anyone else, that parapsychology is an experimental science; that it uses truly objective scientific methods. He has been anxious to make it clear that parapsychology belongs to the scientific disciplines, and that it even adheres to higher standards of experimental control than most behavioral sciences. In a propagandistic manner he uses the expression "The Scientific Method" (TSM). He conceives of this as established and self-evident rules of inquiry. With the help of these, an investigator can easily decide upon what is true and what is not. It is the only way of arriving at truth. It is universal and 'generalized to fit any field of problems'. But it is an 'idealized framework', as in reality you can never achieve this completely. In "The Reach of the Mind" (Rhine, 1947) Rhine gives as examples of principles within TSM ten commandments which one must obey in order to be a faithful adherent of TSM: One must not start with unwarranted assumptions! One must not stick to one's own favorite hypothesis! One must not design experiments which provide no crucial tests for one's hypothesis! One must not draw premature and inconsistent conclusions! Etc..

Of course no serious investigator would object to these general formulations. But there are some very ambiguous terms within the framework of Rhine's rules, e.g., 'unwarranted assumptions', 'crucial tests', the meaning of which has to be analyzed. I believe that the concept of TSM is a common one among most

scientists trained in the positivist tradition. But as Feyerabend (1970) has shown, it is a false concept. It is based on the assumption that irrespective of the nature of reality, or the peculiar characteristics of the investigated phenomena, one can always apply some variant of TSM. One always needs some critical acumen, logical ability, and carefulness, and we can state this in the form of rules, but the way in which one uses them depends on the nature of the problems one faces. There is always an interaction between instruments and properties in the reality one wishes to detect. Thus all methods are based on some assumptions as to the nature of reality. There are no fixed rules or principles which create a demarcation between science and nonscience.

There are inductivistic components in Rhine's concept of science. He believes in "facts", i.e. results reached by way of TSM. These facts are infallible and are true foundations for a structure of scientific principles. One set of facts only admits one conclusion, a conclusion in favour of a certain theory which is said to explain the phenomena. "Empirical data" obtained in an "objective" manner are often regarded as free from interpretation and infallible, as the foundational units of science. Other parapsychologists share this conviction: "facts remain what they are, whether or not they are recognized as such", "facts are facts whether or not they fit into belief systems" (Rao, 1966).

In an interesting paper on "The importance of parapsychology to William McDougall", Rhine explains how the evidence of psi has refuted "the logic of mechanistic biology". Psi experiments afford a crucial test for deciding between a mechanistic and a vitalistic biology. However Rhine does not discuss the problem of many biologists, whom he would consider mechanistic, emphasizing other types of experiments which they regard as evidence against a vitalistic conception (Rhine, 1971).

There are no immediately crucial experiments. The history of science contains an abundance of cases where experiments, with the help of hindsight, have been viewed as crucial in defeating an erroneous scientific theory, e.g. Galileo's kinematical experiments. But only history can determine this, not contemporary scientists.

Eighteenth century chemistry held that a certain substance (or principle), phlogiston, was expelled from materials when they were burned or calcinated. In 1775 the French scientist Lavoisier carried out an experiment in which he proposed to show that phlogiston did not exist; that combustion could be explained by the recently discovered oxygen. However this was what Lavoisier saw in his experiment. Other chemists held on to the phlogiston theory with the help of ad hoc explanations. Priestly in fact, was even able to devise a counter-experiment as convincing as

Lavoisier's in support of the phlogiston theory. Two crucial tests had been carefully carried out and both had been successful, yet they pointed to contrary explanations. The old phlogiston theory was abandoned after some time, but not because of lack of evidence. Rather, it explained too much without being specific enough to allow possible refutation and further, a new concept of science had gained ground in chemistry with Lavoisier, a concept in which quantitative evidence was more important than qualitative. (Toulmin, 1957).

In the Rhinean tradition crucial experiments have been important. But there is no consensus of opinion amongst parapsychologists as to which experiment constitutes the most crucial evidence for ESP, and critics have not been convinced by even the best controlled experiments. It seems that experimental evidence is not enough for the acceptance of unrelated and theoretically unexplained phenomena.

Every field of investigation has a specific territory. In addition, and as special feature in the field, parapsychology faces difficulties greater than most other disciplines as there exists no general agreement amongst the researchers as to the existence of psi, the territory of parapsychological research. The fundamental question is not whether theories or interpretations are valid in parapsychology, it is concerned with the actual existence of the phenomena. It is a unique situation, like denying the existence of matter for the physicist or the existence of microbes for the bacteriologist!

The research plan for Rhine in the 1930's limited his study to establishing the reality of the territory. The main problem was existential: "Are there any psi phenomena?". He did not ask whether all individuals could manifest ESP but whether it was possible to locate it in just one case. The existential question can only be tested in a confirmatory way; it is irrefutable. It would seem that only one strict confirmation will be sufficient to test it, but as we know there are no crucial tests.

The existential question - does ESP exist? - is similar to the ancient query "does the Isle of Atlantis exist?". Suppose we should go looking for Atlantis. We detect a piece of land at sea but it is impossible for us to go ashore and explore it. How do we know it is Atlantis? From its position we can eliminate all other known islands. In this situation we have defined Atlantis negatively (just as with ESP): Atlantis is an island not identical with a, b, c, etc.. We still do not know if it is Atlantis or just a hitherto unknown piece of land. Rhine in (1940, p. 15) thought it possible merely to investigate the existential question without asking anything about its nature. He tried to avoid

unsupported assumptions and presented neutral definitions of his terminology. But how much do you in fact know when you have discovered something which you do not know anything about? The popularizer will immediately accept the mythological connotations of Atlantis and regard it as the discovery of the real Atlantis. When we succeed in investigating the island in more detail, we are unable to do this without presumptions. As Mario Bunge (1967: I, p.178) states, "a problem may be well-formed but its background may be defective or just vaguely indicated".

No question is ever posed without presupposing something. Since there is no question without a background, and since the background may be constituted by falsities or just controvertible ideas, the naive acceptance of a question without examining its background is no better than the naive acceptance of an answer without examining its ground.

Therefore, when on our unknown island searching for the truth of its nature, we ask questions and pose problems, we do this knowingly or tacitly in the context of the old Atlantis mythology.

The occupation of the Rhinean school and of other researchers too, e.g. Soal, Tyrrell, with the problem of the existence of psi phenomena may be the main reason for the common stress on confirmation in testing parapsychological theories. Rhine has always insisted that it is only positive, i.e. significant, findings that count. The editorial policy of the Journal of Parapsychology has disregarded insignificant results, if the study did not contain some innovations of method or other "illuminating observations". "There are obviously too many ways of going wrong in the search for delicate capacities such as ESP and PK for us to draw any conclusions from a failure to obtain significant results", or "all he can say is that under such conditions he obtained no evidence of psi, and there is no reason to publish that" (Rhine, 1950). Just recently Rhine has defended his standpoint in a detailed discussion (Rhine, 1975).

There is a common belief in the Rhinean school that a psi investigator needs two qualifications. He must have the proper methodological training in The Scientific Method. He also needs to be a subtle personality in order to motivate subjects enough to produce psi. Therefore some researchers may never succeed in obtaining significant results. Critics have sometimes interpreted this thus - if one has to believe in psi to investigate it, this believer will be inclined to accept looser experimental conditions.

It is obvious that very good reasons must be given for not publishing insignificant results and this is certainly not the case before the hypotheses of lucky-unlucky investigators have been tested. The Rhinean theory is well worth a serious try. Recent

studies on experimenter effects give it credibility (Rosenthal 1966).

There are several possible reasons for negative psi results. They may arise from bad research work, i.e. psi was present but the investigator could not detect it, or psi was not present because the conditions were not favourable for it. To blame the investigator for not having elicited psi is to expect him to be too much the magician. But if the conditions - and experimenter variables may be included - were very similar to previous ones, the investigator may have refuted earlier results. Insignificant results must not be neglected in a discipline where the significant results are relatively few and unrelated. To consider the possibility of falsification is important, the paradigm is not to weaken and degenerate. It may be wise to be careful with the "facts" one has, but it is also necessary to tolerate refutations.

The common attitude to negative results reminds me of the psychologist who wanted to test the hypothesis: Psychotherapy can cure schizophrenia, an impossibility many psychiatrists would say. He reported success in a paper, where the effects of psychotherapeutic treatment had been quantitatively evaluated. The psychiatrists were quite surprised, but not as regards the results. Obviously their diagnoses were wrong, the subjects could not have been schizophrenics!

Early on Rhine engaged himself with the problem of repeatability. This is a stumbling-block for many researchers and much has been said and written about it both within and outside the field (Murphy, 1971; Rhine, 1954). The critics have demanded a truly repeatable experiment before accepting parapsychology as a science: A specification of necessary and sufficient conditions for the occurrence of psi phenomena, so that any competent investigator can reproduce earlier experimental results with approximately the same outcome. Often, repeatability is discussed together with the general demand for intersubjectivity. But to define objectivity as intersubjectivity is a statistical conception of truth which can be proven false in cases where all investigators repeating or observing phenomena share the same wrong assumptions as to the nature of the phenomena. In that case it is not objectivity but collective subjectivity. In the 18th century many investigators reproduced experiments purporting to show the existence of phlogiston and they were able to specify many necessary conditions for its appearance.

Of course, successful reproduction of psi experiments is desirable, but not for the sake of scientific recognition. Rather, it helps us to predict phenomena and to control them in order to

determine the process of psi itself. It is not the starting point of scientific investigation, but the conclusion. To achieve complete repeatability would imply to have knowledge of the sufficient conditions, and there are a lot of established phenomena in respectable sciences where we are not familiar with all the sufficient conditions, e.g. for pregnancy. The more one understands of the total context of the event the more repeatability one will have.

There is no repeatable experiment in modern parapsychology. In some areas there is some hope of a breakthrough, e.g. in animal psi or in the relation between motivational factors and psi. Some parapsychologists have claimed that it is futile to search for repeatability as psi is spontaneous. Of course the psi process can have randomness as one of its properties, but the fact that we so far have no repeatable experiment does not prove this. Perhaps the phenomena can only be repeated with a certain probability.

Psi phenomena are often described as spontaneous, but this is not identical to irrepeatable. It may suggest that we should give up looking for mechanical explanations and turn to a more teleological mode of explanation. Therefore we have to view the phenomena as purposive, as an expression for a person's life needs. Nevertheless, the phenomena can be controlled, but not in a technological way from the outside.

How do parapsychologists view their field in relation to other sciences? What is the place of parapsychology in the system of sciences? It is evident that the Rhinean school considers parapsychology as a very unique science. It is a border science but also a frontier science. It is the only science which has as its main territory, the Mind. Further as it is a science of anomalous phenomena it is also a revolutionary science. It is the anomalous character of psi phenomena that has caused so many controversies in parapsychology. Parapsychologists have, on the whole, preferred to stress the controversial character of the phenomena rather than trying to harmonize their findings with existing knowledge and values.

Rhine has defined parapsychology as "the study of those phenomena attributable to personal agency which in some degree transcend physical explanation". Therefore, "it is obviously a branch of psychology". He has considered it "only a matter of time until parapsychology is fully integrated with general psychology", an integration in which however, parapsychology will cause a re-orientation of psychology towards the study of mind, not just behavior (Rhine, 1949).

Rhine did not succeed in establishing parapsychology as an academic discipline, nor did he succeed in integrating it within

psychology. He left the Department of Psychology at Duke, and after the foundation of FRNM parapsychology had no formal connections with Duke University. His view of the systematic place of parapsychology also changed following this. In 1967 he summarized the relations between psychology and parapsychology through the century. Most psychologists were still as hostile to psi research as in the late 19th century. Rhine no longer believed in an unification, but stressed the independence of the two fields. Parapsychology has its own territory, its own problems and methods, and it ought to have its own recruitment and training. Parapsychology has become a specialty. In its stress for autonomy it has underrated the importance of close conceptual bonds to the psychological sciences (Rhine, 1968).

The Rhinean paradigm was born in the 1930's at a time when the behaviorist school had attained a dominant position. No doubt Rhine borrowed much of the behavioristic concept of science. He used the S-R-scheme as an experimental model. Parapsychologists started to look for a connection between variations in stimulus inputs (targets) and response outputs (guesses). In his early investigations Rhine used an objective definition of ESP, approximating to operationalization. He demanded strictly objective methods of observation and statistical evaluation. During the 1940's he worked with large groups of subjects and looked for intergroup differences, using averages for each group. These are also recognizable patterns in the neo-behaviorist paradigm of the 1930's.

Of course it would not be fair to label Rhine as a behaviorist. He did not look at behaviorism as an exemplar. On the contrary, William McDougall, his "teacher" and benefactor, was much more the ideal researcher for him. McDougall defined himself as a vitalist in the battle of behaviorism in the 1920's. But we also have to note that he in fact defined psychology as "the science of behavior" and did not oppose the positivist concept of science held by the behaviorists. It was the behavioristic world-picture which McDougall could not accept. When Tolman argued for strict observability in psychology, he also considered concepts like mind and soul to be unscientific: "An organism's private mind, if he have any, can never be got at". So if Rhine was a methodological behaviorist (together with Tolman and Lashley) his concept of science would clash with basic assumptions within his world-picture. He wanted to make parapsychology the Science of the Mind and he tried to verify the concept of mind by objective means. Finally, behaviorists have an empiristic concept of knowledge. But is it logically possible for a parapsychologist to be an empiricist, to think that all knowledge is derived from our senses, and still claim positive psi results? Our concept of science

depends on the concept of knowledge and this in its turn on the knowledge of man. It is possible that the concept of ESP involves some bad connotations; the psi process does not have to be perceptual. Yet if the evidence for psi is valid the informational capacity of man seems to differ considerably from what we are normally inclined to believe. Modern philosophers of science have used some arguments from and been influenced by the image of man developed within gestalt-psychology and psychoanalysis. However, the consequences of parapsychological research for our concept of scientific methodology have not yet been investigated.

Nevertheless, a few parapsychologists have recently questioned the experimental model, where individuals are regarded as closed passive systems manipulated by external factors. "The experimenter can by no means be considered as an uninvolved spectator" (Schmidt, 1974). Parapsychologists should be more aware that the methods they use limit the results or rather, lead to limited aspects of reality. Intentional and motivational aspects of the psi process have, for example, not been studied until very recently. Moreover, a phenomenological perspective may prove a valuable compliment to parapsychological research.

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